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[Aller au sommaire du numéro](#)

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# Comptes rendus

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On several occasions over the last twenty years there have been discussions amongst a small group of involved scientists about whether or not the time was ripe for a synthesis of the great outpouring of research papers on the Quaternary environments of the Eastern Canadian Arctic. Uncertainty resulted in the inevitable, and probably appropriate, delay. Thus it is no over-statement to claim that the present work, edited by J.T. Andrews, is long-awaited. It is something of a relief that Professor A.L. Bloom's suggestion (editor's preface) has borne so much fruit. However, considering that Bloom's suggested "Consolidation" has spread to Baffin Bay and Western Greenland, and that the level of productivity continued to accelerate over the years (Andrew's score alone now exceeds 250 published items!) the task has become correspondingly the more formidable, presenting the serious reviewer also with a not inconsiderable labour. Where, then, do we begin to discuss this impressive volume featuring nearly 800 pages, 26 chapters, four editorial overviews, and 40 authors? Much of the work presented is original, some partially or completely review of previously published work, and the whole represents contributions from individuals and institutions from four different countries.

The aims of the volume are "to summarize work accomplished..., to reflect on the implications, to identify areas of uncertainty and confusion, and to chart research plans for the future... to present a synthesis and an in depth assessment of the Quaternary history of this region" (p. 1). Later in their introduction Andrews and Funder go on to state that "the major aim... is to objectively examine the field and laboratory data in order to present whatever model the authors feel explains most facets of the actual Quaternary record" (p. 2-3). They also state that their intent "is not... to present a series of detailed reviews of previous observations and theories".

This review will attempt to assess how far these objectives have been achieved; it will

also examine in some detail individual chapters that especially interest the reviewer, since space does not permit an even-handed treatment while a work of this magnitude demands some detail and a degree of scientific rigour.

It is some thirty years since I began what can best be described as a geographical reconnaissance of Labrador-Ungava and Baffin Island—a presumptuous undertaking, in all probability, yet one that a graduate student in the 1950s could contemplate partly because of the emptiness of the Eastern Canadian Arctic and Subarctic and the near absence of qualified "seekers", and partly and most importantly because of the richness of ideas, stimulation and encouragement of inspired mentors at McGill University, especially J. Brian Bird, Max Dunbar, F. Kenneth Hare, T.H. Manning, and Sven Orvig, and the existence of the McGill-Carnegie Arctic Research Programme. At that time the area, however defined, was largely unknown—in the sense that available topographic maps (1:500,000 and 1:250,000) had warnings to pilots "highest elevation unknown" and it was necessary to coin place names and process them, and most of this region, larger than Western Europe, had only trimetrogon airphoto coverage, much of it of rather poor quality. But even that was vastly better than conditions faced by R. Bell, A.P. Low, A.P. Coleman, R.A. Daly, and it is well that we are all aware that we walk in their footsteps, even if the imprints, for one reason or another, may have been partially obliterated. And, in addition to the physical conditions, the psychological-intellectual climate of the time must be regarded as tangible. Especially important to my mind was the dominance of the Flintian school of thought based upon hypothesis and on the essentially reconnaissance field observations of N.E. Odell and V. Tanner, and a few others.

Thus it is important that these thirty years of "progress" in our understanding of the geographical basis of this vast region and its related Quaternary record be considered within the context of Andrews's book. On the Canadian side alone there are now over a thousand <sup>14</sup>C dates (against virtually none in 1955), vertical air photographs, Landsat imagery, maps, in places at scales of 1:25,000/1:50,000, and the greater unknown, the land below sea level, is on the verge of becoming commonplace. The "unknown of 1955-1965"

can be likened to walking along the seashore north of the deliberately named *Remote Lake* (northeast coast of Baffin Island, Lat. 70° 58'N.) looking for a Norse battle axe and finding... sea shells.

What did we "know" then, and what is shown, or reconstituted, in this new volume? Is it new knowledge, or the affirmation and extension of "old" knowledge? Were the big questions that were posed at that time answered, found to be irrelevant, or are some of them still unanswered?

All students of the Quaternary in the 1950s were informed absolutely that the Laurentide Ice Sheet at its maximum was a single dome of such immense proportions that it inundated all the high coastal mountains, that it combined with an enlarged Greenland Inland Ice which similarly buried the highest coastal summits along the Greenlandic seaboard. *The Glacial Map of North America* (FLINT, 1945) demonstrated this great truism, and we understood from its reported field observations, for instance, that the marine limit along the northeast coast of Baffin Island stood at 246 m a.s.l.

The only partial ripple in this monolithic structure, which included a model of initiation, growth, and dissipation of the Laurentide Ice Sheet, was the description of a Baffin-Ellesmere glacier complex and a stipulation that, after the late-glacial ice sheet margin had receded across the Arctic mainland coast, recession continued in unbroken and accelerating form until complete deglaciation had occurred (CRAIG and FYLES, 1960).

The questions posed between 1955 and 1965, therefore, included the following, from which other associated and secondary questions quickly sprang:

1. Was R.F. Flint's hypothesis supportable?
2. Could the Scandinavian biologists' adherence to the Nunatak Hypothesis be applied to Labrador, Baffin Island, Ellesmere Island, and Greenland?
3. What was the pattern of land-sea interaction since the last glacial maximum, and could increasing knowledge throw light on questions (1) and (2)?
4. What was the actual pattern of deglaciation?

5. When did Hudson Bay "open" and what was the location of the final remnants of the Laurentide Ice Sheet?
6. Were the apparent differences in degree of rock weathering with elevation significant and how could increased knowledge of this phenomenon assist?
7. What was the significance of the topographic situation of the Barnes Ice Cap and could an attempt to compare accumulating knowledge from Labrador-Ungava and Baffin Island resolve some of the details?
8. Was the Laurentide Ice Sheet a single large and monolithic dome, or were there several semi-independent domes and/or ridges?
9. What was the significance of the "Cockburn moraines"?
10. What climatic conditions were conducive to glacial initiation and what were conducive to glacial dissipation?

This list could be enlarged upon although it is difficult to reconstruct just when these questions were raised.

The work of one's predecessors must be acknowledged; scientific etiquette demands it for obvious reasons, although there are more important reasons. Thus it is highly appropriate that Andrews and Funder (Chapter 1) acknowledge the leadership and long-term endeavour of Dr. Anker Weidick in Greenland and Dr. Robert Blackadar in Baffin Island. It is the more conspicuous, therefore, that the life's work and commitment of Professor Fritz Müller on Axel Heiberg Island and the North Water of Baffin Bay goes unmentioned and that of Dr. Jorge Frisrup and Paul-Émile Victor on Greenland is passed over as well as the more recent challenge posed by Professor John England in the Farthest North. Perhaps more serious, to my mind, is the misrepresentation of the difference in research histories and traditions. On page 9 it is claimed that "the North American tradition has been one of concentrated efforts often dedicated to the solution of specific problems" while work in Greenland is characterized by "systematic and continuous gathering of standard information". It is well documented that the Geological Survey of Canada preceded in a manner strictly similar to that of the Greenland Geological Survey; this especially embraces the work of Blackadar and led to the seminal monographs of Dr. V.K. PREST (1968, 1984). The ultimate example of this is the current works under Dr. R.J. FULTON's editorship (1984, 1987 in press).

It is to be regretted that the importance of Professor F.K. Hare's establishment of the McGill Subarctic Research Laboratory is also overlooked since without it INSTAAR and most

certainly Andrew's Baffin Island program would never have materialized. It is suggested that this oversight is more than the result of a need to conserve space; it amounts to a denial of one's origins!

Nevertheless, Andrews's leadership and dominance in the enlargement of our knowledge of the Quaternary history of the Eastern Canadian Arctic over two decades is demonstrated in masterly fashion by this volume; the West Greenlandic component, though very valuable, is secondary by comparison, and the oceanographic studies, though perhaps promising some of the most important future potential, are in their infancy.

The work is divided into five sections: (1) introduction and present environment; (2) glacial landforms and sediments; (3) Quaternary paleoceanographic and ice-core records; (4) Pleistocene glacial and non-glacial stratigraphy; and (5) Holocene sea levels and climate. Sections 2 to 5 are concluded with an overview or "survey" by the Editor (usually two pages). I will now turn to a series of brief discussions of selected chapters.

#### CHAPTER 4, BY E.K. DOWDESWELL AND J.T. ANDREWS

This chapter is a most interesting discussion of the basic question of fiord formation. It raises the century-old dispute of the relative importance of glacial over-deepening versus tectonic activity, and especially the possibility of equating fiords and graben. The discussion introduces probably the most extensive example in the literature of codifying fiord descriptors and assembles a data bank of impressive dimensions. However, the authors' major thesis seems to be based on the setting up of a straw man—"most modern text books subscribe to a predominantly glacial origin for fiords..."—and yet still having to conclude (p. 113-115) that "The origin of the Baffin Island fiords cannot be resolved by our data, but we suggest that several points discussed earlier must lead to the rejection of a *simple* glacial origin." One must ask, which points?, and suggest that here we have a *reductio ad absurdum* since acceptance of a *simple* glacial origin must have collapsed decades ago. Re-reading SUGDEN and JOHN (1976) and earlier writers, it would appear that no serious researcher in glacial geomorphology of recent years accepts a simple glacial origin. Certainly, there is the general implicit assumption that fiords are the result of glacial modification of pre-existing forms—fault-lines, fault-blocks, graben, and other lines of weakness, initially worked upon by fluvial processes and subsequently modified by glaciers. Additionally, the authors do not even state the coincidence between high latitudes, formerly glaciated high mountain coasts, and fiords,

let alone discuss the implications of this in relation to their description of the Baffin Island fiords.

It would have been useful if the authors had addressed the following points:

- a) That their Group 1 fiords are found in different locations, physiographically, than most of their Group 2 fiords, but that 25 of Group 1 occur within the distribution of Group 2, including one cluster of 6 which locally comprises a majority;
- b) How distinct are Group 1 from Group 2? Some of the differentiation appears to rely primarily on fiord length.

The map (Fig. 4.7) is not very helpful (i) because the actual coastline where the fiords occur is omitted and (ii) because it is disorientated, with no indication of latitude or true North.

This chapter provides a great deal of quantitative data but despite this considerable effort, we are left with the sad conclusions that (i) the question of fiord origin cannot be resolved and (ii) that the Baffin Island fiords fall "within the range of shapes observed in other fiord regions", which just about takes this study to the level of Nansen's.

#### CHAPTER 15, BY R.A. KLASSEN

This "outline of the glacial history of Bylot Island" fills a critical gap in knowledge and provides us with a well-presented discussion of the field evidence supporting a chronology of three foreign glaciations and their relationships with the local ice. These are referred to as: pre-Eclipse (early or pre-Wisconsinan equivalent) which, together with local ice, overwhelmed the highest summits; Eclipse (age greater than 43,000 radiocarbon years); and post-Eclipse (Middle or Late Wisconsinan equivalent) during which foreign ice was largely confined to the channels around Bylot Island but in places extended more than 50 km beyond the "Cockburn moraines". This forces an appropriate comparison with my own identification of high-level glacial erratics in the Inugsuin-Clyde fiord and Torngat regions and illustrates the danger of the use of negative information to support the contention that some of the higher summits were never covered by ice (e.g., BOYER and PHEASANT, 1974). Some local glaciers extended up to 10-20 km beyond their present positions; present glaciers are at or close to their Neoglacial limits.

#### CHAPTER 16, BY MICHAEL KELLY

This is a review of the Quaternary geology of western Greenland. It is a vital piece of the jigsaw puzzle but is extremely hard to read and contains much stated uncertainty.

The broad conclusions are that all the higher coastal mountains were inundated and that the outer limits of the continental ice sheet reached the edge of the shelf (Hellefisk Moraine), yet the chronology is uncertain. Table 16.2 (p. 483) illustrates the quandary facing northern stratigraphers but to give us the Iv-nârquigsorrsuaq and Eqlugsugssuit amino-zones is a little tedious.

The main problem with this chapter is that the reader must progress through labyrinthine discussions of stratigraphy and correlation with a small area of conviction with which to conclude. This is the more perilous as the following example demonstrated. On page 485 we learn that "ages are calculated according to the Arrhenius equation for the total alle:lle ratio (Miller, pers. comm., 1983)." This must impress and challenge the intellect of all but the senior priesthood; but for the mere reader it becomes rather meaningless and confidence becomes a scarce commodity when we read on, "Although this assumption is probably not valid..." and further that "...deposits belonging to the Olrik Fiord, Kaf-feharn and Lakesbugt local amino-zones may be from one or two marine events..." and still further on that "...Kaffeharn and Lakesbugt marine events therefore may or may not be the same event." The end point in this discussion is that "A Middle Quaternary age is possible for this event"—is it not also possible that the material in question, that is so difficult to date, simply fell off the moon! At least the locations of Iv-nârquigsorrsuaq and Mudder-bugt could have been provided with a degree of geographical certainty.

The author appears very confident in his unqualified reference to "erratics" which he stipulates "occur on all the outlying islands and the highest summits..." but his Figure 16.2 certainly does not indicate erratics on "all...the highest summits" and my thirty years of erratic experience have prompted me to a degree of caution.

#### CHAPTER 18, BY L.E. OSTERMAN, G.H. MILLER, AND J.A. STRAVERS

The authors outline their ideas about the late- and mid-Foxe Glaciation of southern Baffin Island, which should be read in close association with Chapter 19 (in my opinion it would have been better if the two chapters had been edited and combined).

As we have come to anticipate, there is a great amount of detail and potentially controversial interpretation, if not actual speculative "correlation". A point of great interest (p. 526) is the authors' interpretation of a limestone-rich glacial marine sediment on Loks Land, dated at "greater than 40 ka", as being deposited by a "major mid-Foxe expansion of the Labradorean ice dome". Considering

the important implications of this interpretation the unexplained dismissal of the alternate explanation—deposition by an outlet glacier flowing down Frobisher Bay from the north-west, is the more surprising.

Pages 526-527 provide a useful reference to the "non-glacial landscapes" of northeast Blunt Peninsula and the higher parts of Loks Land, yet make no mention of the comparable landscapes of the strategically placed Lady Franklin and Monumental islands (IVES, 1963).

Characteristic of the involuted discussions of this and some of the other chapters is: "a recent tandem accelerator datum of  $8,452 \pm 375$  near the same depth suggests the chronology may have to be revised" (p. 531). What is a "tandem accelerator datum"? and where does the prospect of further revision lead us, or, put another way, how many of the current preferred explanations-correlations are we to take seriously? This question may seem spurious yet we read a little later: "It is our belief that radiocarbon dates on organic carbon in [marine] cores provide necessary intra-core correlations that may or may not be chronologically correct." Again: "This paper follows the chronology... when in actuality it may correlate instead with the later Cockburn advance." And "A C-14 date of 27,255...on organic matter...suggests the core spans the last 30 ka...although the alle:lle ratios...from 260 cm depth...suggests the core may only span the last 18 ka", which raised the spectre of aminostratigraphy without normal geological stratigraphical constraints, discussed below. The problems for the reader are compounded when we go on to read that (p. 534) "Evidence for this glacial advance, here termed the Kneeland advance, is preserved only in core 156" despite the fact that no associated moraines have been identified since they are presumed to have been destroyed. It requires a great deal of blind faith to accept the need for a "Kneeland advance" on the basis of such flimsy data. Credibility is further strained (p. 539) when in the context of citations to MERCER (1956), BLAKE (1966), MÜLLER (1980), STRAVERS (in prep.) we are presented with a "current consensus". How can there be a consensus across this time-span of publications?

It is curious that on page 540 there is a reference to the asymmetric retreat and ice-divide migration of the "Foxe Basin" dome, credited to ANDREWS and FALCONER (1969) without reference to IVES and ANDREWS (1963) and FALCONER *et al.* (1965). What is the evidence for a "regional Amadjuak Lake centered Frobisher Bay lobe"? (p. 541) and why not cite the original references for deglaciation of Hudson Strait and Hudson Bay at  $8,000 \pm$  BP (p. 543)?

#### CHAPTER 21, BY J.T. ANDREWS AND G.H. MILLER

This chapter discusses "Holocene sea level variations within Frobisher Bay." Again, characteristic of many other chapters, the authors introduce a great deal of welcome new data but, in their discussion confuse rather than enlighten. For instance, on page 593, there is a statement that on the eastern tip of Meta Incognita Peninsula the marine limit lies at about 40 m asl and dates from about 8600 BP; whereas the following paragraph informs us that "toward the outer coast the 20 m marine limit at Nannuk (spelt two ways) Harbour is associated with...26,000 BP", yet there is no attempt to explain this apparent contradiction. MERCER's early work (1956) is quoted in the next paragraph and yet the claims by Mercer for marine limits in excess of 400 m a.s.l. are not discussed. Page 595 provides the following gem... "that for a grounded ice sheet flowing over substantial bodies of water, the uncompensated isostatic depression is approximately the mass of ice that extended above sea level." How does a grounded ice sheet flow over substantial bodies of water? Perhaps this is why we must reject the simple glacial hypothesis of fiord formation!

While the points cited may be brushed off as trivia, in many of the INSTAAR papers the attendant confusion from lack of attention to detail, especially geographical detail, is not just irritating, it does justify an often heard claim that "more dark has been thrown on Baffin Island". We have to struggle with the north coast of Frobisher Bay which actually trends NW-SE and the east coast of Baffin Island, which also trends NW-SE, with place names misspelled, with maps and diagrams disoriented, a problem exacerbated by the INSTAAR practice of omitting the indication of true North, in many instances of omitting latitude and longitude, and in others of placing them down one side of the diagram only so the reader cannot determine "which way is up". To complete this level of confusion the place name "Barnes Ice Cap" is not shown, nor that of the "Penny Ice Cap", despite multiple references in the text to these important geographic features. It is perhaps not surprising that the new quantitative and "scientific" Baffin Island program passed out of the hands of simple geographers. But if we do not know where we are geographically, do we really know scientifically? In contrast, the maps and diagrams of Chapters 6, 7, 8, 9, 10, 13, 15, 16, and several others are good to excellent.

#### CHAPTER 26, BY L.D. WILLIAMS AND R.E. BRADLEY

This is a refreshing and highly appropriate final chapter which provides a first rate sum-



mary of the paleoclimatology of the Baffin Bay region. It goes a long way toward tying together many of the otherwise disparate sections of the book and leaves us with a well-stated future task.

"The question of the synchrony of climatic change even within the eastern Canadian Arctic-southern Greenland region remains unresolved, much less that over the entire Northern Hemisphere or globally (cf. Denton and Karlen, 1973)."

#### AMINOSTRATIGRAPHY OF SHELL-BEARING DEPOSITS

In Chapter 14, Miller provides a much needed service by explaining the basis of aminostratigraphy. Furthermore, he provides a courageous explanation of some of the earlier confusion deriving from its application...lack of adequate laboratory control and especially the realization that contamination occurred during sample preparation. The systematic correction and provision of "better" dates to scientists who had paid for and published interpretations on processed earlier dates clears up many problems. Nevertheless, it does emphasize the difficulty with which many of the contributors to this book are grappling—uncertainty and miscorrelation. While my own single experience may be of little moment today, I still have no explanation for why seemingly "old" mollusc shells from the Northern Labrador coast were not only "aminozoned" as Holocene but that I was recommended not to "waste money" by having a sample radiocarbon dated. It was fortunate that my non-scientific intuition prompted me to ignore this advice because the ensuing  $^{14}\text{C}$  date was  $43,000 \pm$  and represented the molluscs as the first find of its kind on the Labrador coast (IVES, 1977).

This only serves to illustrate the challenge of proceeding into the "correlation unknown" which many of the authors of this book have appropriately accepted. However, it may partly explain the frequent uncertainty and apparent contradictions and ambiguities contained within. It also cautions us that there is an inherent danger when aminostratigraphy becomes separated from lithostratigraphy; it also raises the question of precise sample collection and identification in the field, as for instance with the rumoured inversion of peat monolith sections of the infamous Windy Lake core (p. 146).

Andrews's section reviews (surveys) are flimsy and disappointing to my mind. They do not fulfill the stated objectives nor set the goals for continued future effort. There is not even an attempt to compare the western Greenland and Baffin Island terrestrial stratigraphies. In this sense the book falls short of its aims and an opportunity has been mis-

sed. While some of the individual chapters are extremely important and authoritative contributions to knowledge, the whole is rather 26 more papers to be added to the 500 or so other papers, with all of these internal inconsistencies and contradictions, so that we must conclude that, with some singular exceptions, "more dark has been thrown on Baffin" and the surrounding region. The extension of the "consolidation" to western Greenland at the apparent expense of a proper consideration of Labrador-Ungava is an organizational weakness of some moment.

In conclusion, a word about mechanics. The book is well bound and has an attractive cover. Many of the chapters, and especially chapters 1-5, have been produced from poor camera-ready copy with numerous typos: om, av, An, em, Ho, am, and most upper/lower case combinations run together and (especially Chapter 5) give a first impression that ink has been splashed across the page. The lack of a justified right margin gives an ugly impression, but missing lines and phrases occur and many errors have been allowed to stand. Many of the diagrams and maps are very poor: place names, true North, latitude and longitude are missing, misplaced, or in error. Three typos were detected in the instance of a single caption. And as an example, Figure 3.6 (p. 81), discussed extensively in the text, has several of its ice streams incorrectly labelled—or labelled differently in the text.

The absence of a good map with place names and latitude and longitude and the Arctic Circle, and the lack of a map showing present ice cover are surprising and damaging omissions. But the degree of error in some of the chapters cannot but raise the question that inattention to accuracy and detail possibly pervades other areas of work and leaves us with a feeling that we have US \$75 of flawed publication that is in urgent need of a serious editing job. We are left still waiting for the eventual synthesis!

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